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THE TEACHING OF MATHEMATICS TO STUDENTS OF ENGINEERING[1]

FROM THE STANDPOINT OF THE PROFESSOR OF ENGINEERING

I feel that in this discussion we engineers occupy rather an unfortunate position, on account of the fact that we are compelled to assume the position of critics. The student comes to us from the teachers of mathematics, presumably equipped with a knowledge of that subject, and it becomes our duty to teach him subjects in which he makes use of this preparation, and to find out whether he has learned to use mathematics as a tool. However, I believe that only by friendly criticism can progress be made, and that every one ought to be willing to accept such criticism when given in the proper spirit. I had much rather be criticized than criticize others, and we teachers of engineering hope that we are always ready to receive suggestions, not only from other teachers, but from practising engineers.

I must first insist that for the engineer mathematics is to be regarded as a tool—not as something which is studied simply for the development of some mental powers, but for the ability which it ought to give a man to *do* something—to use the results and methods which he has been taught in solving the problems of his profession.

There has been a good deal of discussion in the past as to the value of mathematics simply as a means of mental training, without reference to its use, and perhaps most of us remember the paper by Sir William

[1] Continued from the issue of August 7.

Hamilton written seventy-six years ago, in which he maintains that there is no one of the subjects in the curriculum which develops a smaller number of mental faculties or develops them in a more imperfect and inadequate manner than mathematics. I have never seen what has seemed to me a conclusive refutation of Sir William Hamilton's main arguments, and for my part I am disposed to agree with him in general, and to assign a comparatively low value to mathematics simply as a training, aside from its applications. I have not observed that students trained in this subject are able *to reason* any better than students who have ignored mathematics; indeed, I believe that many non-mathematical subjects afford a better training in reasoning than the study of mathematics. This view may perhaps be justified by remembering that mathematics, aside from geometry, deals with questions of quantity and number, but not with questions of quality. The student puts certain fixed data into his mathematical machine and grinds out the result. He does not learn to observe and to discover the finer and more elusive, but equally important, sources of error likely to occur in the ordinary questions of daily life, because he is dealing with a rigid, unyielding, logical machine. In this way his mind may become hardened—he deals with rigid demonstrations and is unwilling or unable to appreciate or submit to a less rigid method, which is often the only possible one. The best student of mathematics is frequently one of the poorest of engineers. Give him fixed data and he will get the proper result, but he may be entirely incapable of attacking a practical problem, or of deciding what the proper data are.

I have not observed that students of mathematics are, as a rule, more *accurate* than other students, or that a training in the branches of mathematics above arithmetic leads to accuracy. Indeed, it more often appears to pervert the sense of perspective, and to lead students to work out a result to several figures in cases where a smaller number only may be significant. Mathematics does not train the *observation*, neither does it train the *imagination*, except in the geometrical branches, which are now comparatively neglected since the powerful modern methods in analysis have been introduced.

Hamilton only allowed, as I remember, that mathematics adequately trained one faculty, namely, that of *continuous attention:* but I fail to see that this is trained any better by the study of mathematics than by that of language, chemistry or by other natural sciences. Unfortunately, as at present taught it does train the memory, in a way that it ought not to do. The ordinary student of mathematics subordinates *perception* to a *memorization* of formulæ and rules.

I believe, therefore, that from the point of view of the engineer, mathematics should be taught with the object of giving the student power to use it as a tool. With reference to this I think it is fair to say that the consensus of opinion among engineering teachers and practitioners is that the results of the present mathematical training are very poor. The average student who has completed his mathematical course is frequently quite helpless when called upon to attack a concrete engineering problem, and it is a common remark by civil engineering students that they did not really learn any mathematics until they studied mechanics or the theory of structures. The results seem to be almost equally poor no matter what institution the student comes from, for in my classes there have been students from most of the principal universities and technical schools in the country and I have failed to notice any great difference in them in that respect.

They very generally lack the power to *do anything* with the mathematics which they have been taught.

With reference to the reasons for this state of things, I venture to state what seem to me to be some of them, and the suggestions which have occurred to me by which possibly the results might be improved.

1. In one of the previous papers a statement was made that many students who studied advanced algebra in the technical schools had not studied algebra in the preparatory schools for the two years previous. This illustrates what I believe to be one failing in our so-called system of education, namely, the lack of continuity. The remedy is to reform and simplify the curriculum, and to unify and simplify the entrance examinations to our colleges and technical schools. So long as these entrance examinations are so extended and cover so large a range of subjects, our preparatory schools will be unable to carry out their true purpose, which is, as it seems to me, no less and no more than that of all education, namely, to train a man *thoroughly* in a few things and to give him the power to do some little thinking for himself and to take up new subjects without assistance.

2. The great inherent difficulty which teachers of mathematics as well as teachers of every other subject meet with is the attitude of the student, and his inability to realize the seriousness and the importance of his work. I am fond of expressing my view in regard to this by the statement that the school is not a restaurant, but a gymnasium; not a place where a student comes to be filled up, but a place where he finds apparatus and the instruction, by making use of which he may strengthen his mental muscles.

The manufacturer can take his raw material and shape it into the form which he desires. The raw material of the teacher is the student, but the teacher can not take this material and shape it; he can only show it how it can shape itself. I believe, however, that much may be done in impressing upon students the proper attitude which they should take toward their work, and by a proper cooperation between teachers and parents, which is unfortunately lacking as a rule in this country, and the responsibility for which must largely fall upon the parents.

3. I believe that one cause of the poor results in mathematical teaching is that too great a stress is laid upon *analysis*. Mathematics is, of course, divided into geometry and calculus, using the words in their widest sense. Geometry is concrete; and the mind perceives the steps in a geometrical demonstration. This branch, the oldest branch of mathematics, however, has been largely supplanted by the modern analytic methods which have been developed during the past three centuries, largely to the detriment, it seems to me, of the educational results obtained. Analysis is abstract—it is a powerful machine, an invention for doing certain things. Into one end of the machine we put the data; we turn the crank, and the result comes out with absolute correctness so far as is warranted by the data. Now I believe that too much stress is laid on these analytical processes; that the student is not urged to visualize his results, to express them geometrically and to interpret his equations. I warmly second the remarks of Professor Ziwet with reference to descriptive geometry, which I believe should be treated as a branch of mathematics and taught more thoroughly, as it is taught in Germany. For my part, I derived as much benefit from my study of descriptive geometry, and afterward from the study of projective geometry, as from any other mathematical studies. These studies train

the imagination, which analysis does not do. But in the use of analysis, the first step, namely, the formulation of a problem, is really concrete. This, too, is neglected in our usual courses. Our examination papers are full of questions which involve simply the analytic processes—the differentiation, the integration, the twisting and turning of equations, while much less attention is paid to the formulation in mathematical language of practical problems. Our students, therefore, when they meet a practical problem, are unable to select or judge of the correctness of the data, and even if they can do this, are unable to formulate the data as a preliminary to the solving of the problem by the use of the mathematical machine.

One of the great defects which I find in students of mathematics is one already referred to, namely, that they do not *interpret their equations.* The average student who has completed his mathematical course, for instance, has not the slightest conception of what a parabola is. I make this statement advisedly, because I have tested it again and again for years. If he could tell you what a parabola really is in his mind, he would probably tell you that it was a curve of more or less beauty represented by letters. Perhaps he could tell you what the letters are, but give him a concrete problem and he would convince you immediately that he did not *know* what the letters mean.

4. Another defect, as it seems to me, in our present methods, is the lack of training in mental operations. In the good old days *mental* arithmetic was taught, but that seems to have gone out of fashion, with so many of the other good old methods. Ask the ordinary graduate of our mathematical courses to tell you the square of 20.75 without using pencil or paper and he will look at you openmouthed with astonishment, but if he had really grasped the meaning of the binomial theorem and had learned to do a few "sums" in his head, any grammar-school boy would, of course, be able to give the result immediately.

5. Another reason for poor results is, I believe, inadequate class-room methods, and especially the use of the lecture system. In Germany, where the students in the universities have had the advantage of a thorough preliminary training, they may be able to appreciate lectures on mathematical subjects, although I doubt even this in the case of the average student. For students in our American universities, however, I believe that lectures in mathematics are almost useless, except for a very small number of students; and yet, I am told that even in some of our high schools mathematics is taught to a considerable extent by lectures. The lecture system is easy for the teacher. It involves no cross-questioning, no endeavor to discern what is going on in the student's mind, no adaptation of question with the object of putting him on the right track.

Again, some mathematical exercises are conducted by sending the students to the board, each with a problem to solve, and then marking that on the correctness of their work. Occasionally a formal explanation of his problem is required of the student. This, again, seems to me to be a mistaken method. Many a student can go through a demonstration of a principle, or solve a problem by substitution in a formula, while knowing nothing of the real meaning of the subject. In my opinion class-room instruction should be conducted by the Socratic method—by question and answer—the teacher endeavoring to put and keep the student upon the right track by showing him what he can do for himself if he will only learn how.

6. Reference has been made to the kind of teachers of mathematics. Personally I

believe that in teaching the subject to engineering students the best results would be obtained if the teachers were engineers, or at least if they were near enough to being engineers to take an interest *in the concrete problems themselves* as distinct from their solution. If I am correct in the belief that mathematics should be taught as a tool, then it can be taught best by those who know how to use it as a tool. Unfortunately, however, it is difficult to get engineers who are sufficiently interested in mathematics and sufficiently masters of that subject, who are willing to devote themselves to teaching. The men who are interested in the problems prefer to devote themselves to those problems, and to go into practical work. It is not necessary, however, as suggested above, that the teachers of mathematics should be engineers if only they will take an interest in the problems themselves, and in the point of view which the student should take. They can do this by cooperation with the engineering teachers, by attending engineering courses, and, perhaps, by a little more realization than they now have that their work is preliminary to other and more important work, and that as a matter of fact if the engineering student does not learn to use his mathematics as a tool it is practically of no value to him. For the engineer, mathematics is the servant, and the mathematical teacher should aim to teach the subject in such a way as to obtain as nearly as possible the results which intelligent engineering teachers and practitioners desire to have obtained.

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FROM THE STANDPOINT OF THE PROFESSOR OF MATHEMATICS IN THE ENGINEERING COLLEGE

We must not take too seriously what engineers have to say in an educational discussion, nor take too much to heart their views on the mathematical curriculum. Practising engineers are not in the habit of thinking very continuously on any educational question, although, of course, they must not confess inability to respond when they are called upon for pedagogical opinions. Every practitioner in the law would doubtless express views concerning legal education if summoned to do so, but he would be a rash educator who would attempt to follow their advice without much circumspection. I, myself, prefer to judge of the engineer's views upon educational matters by studying his actions rather than his words. The things engineers "do" may be taken as a true expression of their deliberate judgment—what they "say" is often ill thought out and in contradiction to their deeds. I therefore prefer to judge of the present needs in the mathematical instruction for engineers by the actual tendencies that I observe in the evolution of technology itself.

What are the great changes that the engineering profession has made in technical science in this country in the last quarter of a century? The changes are quite obvious and not difficult to state. In former days engineering technology was founded chiefly upon current practise rather than upon established principles; it was more closely allied to the crafts than to science. Not only is that day past, but it is no longer the case that technical science looks entirely to pure science for its fundamental material. It has so grown that it is investigating for itself and, in greater and greater measure, developing the basal principles for its own needs. There are very few American treatises in pure science which will compare in scientific thoroughness with several treatises which have lately issued from the engineering press. This is a very hopeful sign in the growth of knowledge—to see applied

science and pure science approaching each other at numerous points, so that it is increasingly difficult to distinguish any line of demarcation between them. In this change, *science is not sacrificing any of its strength nor compromising its ideals.* It is *technology* that is changing—that is becoming less empirical, more systematic, more quantitative, more scientific.

With these well recognized changes in applied science before us, what should be our attitude toward the mathematical science that is necessarily associated with engineering education? What is technology really requiring of the basal sciences? Judging the engineers by their acts and not by their words, what is the real demand that they are making of the physicist, of the chemist or of the mathematician? Is the demand to teach physics or chemistry in this or that particular way, or is the demand of a profounder and more radical sort? The most superficial observation shows that the demand is of the latter kind. The engineer in this twentieth century is saying to the physicist, and chemist, and mathematician: "Know more science. Discover more facts in electricity—in light—in all properties of matter. Give to the world more men like Kelvin, Hertz, Helmholtz. Fill the shelves with ten times the knowledge we now have." These words more truly express the real pressure that engineers are putting upon workers in pure science, than do the words they have uttered in this discussion. As a single example, note that the great electrical and other manufacturing companies are impatient at the rate at which pure science grows, and large sums are spent by them each year in the search for new truth and in filling up the gaps in existing knowledge.

The real demand of the engineer is not for better instruments or tools with which to do his work, nor is the demand for more difficult projects to test his skill, nor even for more capital with which to construct them. The real demand is for more knowledge, more science, and for more of the spirit of science in technology and in technical education. I take as my text a saying of Ostwald: "*Science is the best technology.*" If we teach a trade and not a science the time is largely wasted. If we teach *dyeing* and not *chemistry*, the graduate is already out of date when he begins his career, and he has not the fundamental principles wherewith to bring himself abreast of the times. I therefore regard it of greatest importance that mathematics be taught to engineering students with real enthusiasm for the science itself. It should be taught by men who themselves are actively contributing to the growth of mathematical science. The present spirit of engineering science is such that no instructor in any of the basal sciences is satisfactory who does not see that it is his duty not only to teach what is old, but to be interested in and to take an active part in the development of what is new.

I regard of secondary importance the particular things we do in the mathematical course in the engineering school. Different instructors, equally successful, will have different opinions. Various changes and improvements have been tried at various institutions. At the University of Wisconsin we have made innovations whenever we thought it best, but I regard them all of secondary importance to the first requirement of all, namely, that we demand the right sort of teachers, and that the teaching be done in the right sort of scientific spirit.

The only imperative requirement put upon the mathematics in engineering schools that does not rest as heavily upon the mathematics of the ordinary college course is the demand for compactness. It is possible that there is some room in the

courses in colleges of pure science for the whims and fads of the various instructors, for at some later place in the course the balance may be restored. This, however, is not true in a school of engineering. There is very little room for the practise of fads and new schemes. It is easy to exaggerate the need of a special sort of subject matter in mathematics and a special class of problems for engineering students. We are apt to make some very foolish mistakes, if we undertake to change too freely the scientific material that is presented to engineering students. A good engineer is worthy of the best science and the best instruction that can be brought to him—he himself would be the first to object if a different program were carried out.

I have had a little experience in employing engineering graduates in engineering work. In the past ten years I have given employment, in various capacities, to about one hundred and thirty engineering graduates. This work has been scattered over quite a wide territory and the men have come from the institutions of the east, from the Pacific Coast, from the Mississippi Valley and from the south. I have been able to judge within the limits of my experience what the young engineering graduates know, and what they have forgotten. I find it true that the boys have forgotten a great deal of the material they had in college, and that they have remembered other things. They remember the manual and the mechanical things—how to swim, how to ride a horse, how to fish, how to play ball, how to run the level, how to work the plane table, and how to do stadia work. Now what have they forgotten? The men have forgotten the intellectual things—hydraulics, electrical science, thermodynamics, etc. The human mind possesses an unlimited capacity for forgetting. But my experience shows that the young men forget their hydraulics just as quickly as they forget their mathematics or their mechanics. The engineer in the field observes that a boy remembers the right end of an instrument and seems to be amazed that the same man does not know the right end of an integral sign. He therefore concludes that the mathematics has not been "taught right." If he will compare intellectual things with intellectual things he will find that a miscellaneous group of engineers will pass as good an examination in mathematics ten years after graduation as they would pass in thermodynamics or hydraulics.

It grates on me to hear mathematics spoken of as a tool. Mathematics is to the engineer a *basal science* and not a tool. The spirit of that science is of more value to the engineer than the particular things that can be accomplished. The engineer need not be a mathematician, but he needs to think mathematically, and, to my mind, he needs the power of mathematical thought more than skill in manipulating a few mathematical tools in mechanical fashion. There are already too many factory-made products turned over to the college by the secondary schools. I make a fundamental contrast between the engineer with his mind endowed with the power of creative and rational design, and the artisan with his hands equipped with tools for physical construction. A great engineer must be trained in correct seeing and thinking, and must have the power of reasoning concerning some of the highest abstractions of the human mind. In this aspect mathematics is not a tool—it is a basal science.

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At the close of Professor Townsend's address he urged the desirability of technical schools offering more elective ad-

vanced work in mathematics. It may not be out of place, therefore, for me to call attention to the fact that in the Massachusetts Institute of Technology we have offered and given, among others, the following courses: advanced calculus, vector analysis, fourier series, least squares, theory of surfaces, theory of functions, elliptic functions, hydrodynamics and differential equations of mechanics and physics. Some of these subjects are required in one or more of our courses, but not in any one of the larger engineering courses, which are taken as the basis of Professor Townsend's tables. This elective work, therefore, while valuable in many respects, is not the main work of the mathematical department.

The mathematical teacher is in the engineering school primarily to teach to students of engineering the amount of mathematics which is necessary to them for the proper understanding and practise of their profession. The object is to give the student a grasp of mathematical concepts and processes through their use, as one learns grammar by speaking a language. Hence there is no place in the required mathematics of a technical school, nor indeed in the first courses in a college of liberal arts, for the refinements of modern "rigor." At the same time there should be no patience with a loose or unscientific presentation of first principles. The teacher himself must be thoroughly conversant with modern thought, else he will teach falsehood for truth, and must be enthusiastic in his interest in his subject, else he will fail to inspire his pupils. Hence the teacher of mathematics should be primarily a mathematician and not an engineer. It is hard to find an engineer who has any knowledge of mathematics other than a small fragment which he habitually uses, and any elementary teacher whose instruction goes to the very limits of his knowledge is sure of failure. It may, of course, be possible to superimpose a mathematical training upon an engineering one, but in that case the engineer becomes a mathematician and my contention that mathematics should be taught by a mathematician is not invalidated.

On the other hand, the mathematician should know something of the uses to which an engineer wishes to put mathematics. For that reason such meetings as this are helpful, but I must confess to feeling a little disappointment in not obtaining from the engineers any new light on the concrete problem which confronts the teacher of mathematics in an engineering school. I have met the same disappointment elsewhere in similar meetings. It has happened, elsewhere if not here, that engineers will tell the mathematicians what and how they should teach, in apparently total ignorance of the fact that what the engineer promulgates as a new gospel has been the commonplace thought of the mathematician for years. This ignorance may be due to the fact that the engineer remembers his own training of twenty or thirty years ago and does not know that improvements have taken place. That such is the case may be seen by a comparison of modern with older text-books. Such criticism from the engineers is amusing, but another kind of criticism is not. I refer to the kind which seizes upon the failure of a student to have learned mathematics thoroughly as evidence of poor aims and inefficient teaching of the mathematical instructor. We all know that students pass through our classes and graduate from our schools whose attainments are not what we wish, but while the mathematical teacher delivers his product to the engineering departments and hears of his comparative failures, the engineering professor delivers his product to the world and rarely hears of the specific blunders of his stu-

dents. Another unfair criticism is sometimes heard from the professor of engineering who says that students can not use their mathematics, when the truth is they have simply forgotten some particular fact, formula, or process, which is a fad of that professor. It is unfair to test mathematical training by tenacity of memory or mere quickness in reasoning.

I have said that we must teach our students to use their mathematics. Now in the application of mathematics to a concrete problem there may be distinguished three steps:

1. The interpretation of the data of the problem into mathematical language.

2. The formal operations upon the expression or equations thus obtained.

3. The interpretation of the results back into the terms of the original problem.

The first and third of these steps are really the most important, but there seems to be a popular impression that the second comprises the whole of mathematics. This impression is doubtless responsible for some criticisms of the educative value of mathematics. It is true that relatively a great amount of time must be spent in the classroom in teaching the mechanical processes involved in the second step, and many students in school and college get no farther. To object to the amount of time spent in this way and to demand, as some do, that we confine our time to teaching general principles and applications is to talk as sensibly as a fond mother who objects to a child beginning his musical education by playing finger exercises instead of tunes. The technique of mathematics must be learned first, but the student who never gets beyond the technique has not learned mathematics.

The teacher of mathematics should, then, use all possible means of teaching the first and third of the above steps and should bring his pupils to think of them as the real thing. For that purpose he should seek for applications and illustrations from as wide a range of subjects as possible. He will find himself handicapped, however, in using many problems of real scientific or engineering importance because of the ignorance of his pupils, especially in the first year in the technical school. To illustrate a new mathematical principle by an application to a science with which a student is not familiar is to befog and not illumine the subject. Hence there is something to be said in favor of some of the much-criticized problems of the older textbooks. To my mind a problem is successful if it causes the student to take the three steps just enumerated and is couched in terms familiar to the student, even though it may not be "practical." On the other hand, a type of problem lately coming into use, in which the student is given some formula from a science of which he knows nothing, and is asked to find, say, a maximum value, is as fruitless as if the problem were stated in terms of x, y and z, unless it may serve to convince a sceptical student that the matter he is studying has some practical application.

And this leads me to the most important thing I have to say, and that is that after the mathematical professor has done his utmost to teach the use of mathematics the engineering professor must take up and complete his work. I doubt if any one really learned the use of mathematics in a first course. Facility in using mathematics comes from actual use and not from the solution of illustrative examples. In the course in mathematics the student expects his problem to be solved mathematically and has his mind alert to find the solution, and that too with mathematical principles fresh in his mind. In a course in engineering, his point of view has widely changed. The practical problem has now his main interest, mathematical concepts

are in the background, and he often fails to see the possibility of using mathematical principles until he is trained to do so by the professor of engineering. If the professor, through lack of knowledge or lack of interest, avoids the use of mathematics, the student will soon lose the little he has learned.

In other words, the mathematical training of a student is not complete when he leaves the department of mathematics. It is possible that better results could be obtained if the mathematical department had more time, say for a course in applications of mathematics to miscellaneous problems. But, as a rule, in our technical schools the department of mathematics is allowed barely time to teach the necessary technique with what illustrations and applications can be squeezed in. Hence the mathematical department delivers to the engineering department an unfinished product and it is the engineer's duty to teach the student to use the mathematics he has learned. Unfortunately, the professor of engineering is too often a poor mathematician and avoids this duty.

One of the hardest things a student has to do is to combine two different domains of knowledge, each somewhat unfamiliar, so that he may work freely in both at once, using each as a help in the other. It is this difficulty which makes analytical geometry traditionally hard, and which the student meets again when he studies any form of applied mathematics. It is partly to help overcome this difficulty that we have just made a rearrangement of our mathematical instruction in the Massachusetts Institute of Technology. We no longer have courses in algebra, analytic geometry and differential and integral calculus, but have combined these into one "course in mathematics" extending through two years. Into this course the elements of analytic geometry and of calculus are introduced early and continued late. We hope thus to give these principles more time to become completely domiciled in the student's mind. We have also been enabled to carry out two principles: the first is to introduce no subject until some use is to be made of it, and the second to handle each problem by the method best adapted to it, rather than by the methods of the particular branch of mathematics which one might at the moment be studying under the old classification. We hope in this way to increase the efficiency of our mathematical teaching.

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The program shows three standpoints from which discussion is to emanate. I occupy no one of them. It is true I have had some engineering practise, but I can not be termed a practising engineer. I have had charge of mathematics for engineering students in two engineering colleges, but for nearly a decade now I have not met students in mathematics; and, indeed, I have taught, all told, but an insignificant amount. I am in somewhat close touch with engineering students, but they belong to a particular field, namely, mining, which is possibly less dependent on mathematics than are other branches of engineering. My view-point is, therefore, somewhat of a compromise or average of the three specified in the announcement.

The present discussion seems to me significant. It may bring forth results. In fact it seems to have had some immediate consequences. Last evening after the dinner I heard a very clever mathematician admit that he felt really humble, and I heard a well-known engineer say that to his great surprise some mathematicians had a human side. I asked a pure mathematician sitting near me to show me his hu-

man side, but he only shrugged his shoulders. Perhaps he was not yet sufficiently humbled.

This occasion appears to me to be significant, but as showing conditions which exist rather than as forecasting future changes. It is a symptom of the approach —the arrival, perhaps—of healthful conditions rather than a cause. It may, of course, in its turn become a cause, and operate toward good results. That is not so certain. At the moment it indicates conditions surrounding the teaching of mathematics to engineering students, including the relations between the teachers of mathematics and those of engineering which have been the growth of many years. Those young and virile gentlemen whom we all delight to honor, the Woodwards, have been striving for decades to bring about a closer relation between the teaching of mathematics and the subsequent study of practise of engineering. Ten years ago at the Toronto meeting of the Society for Promotion of Engineering Education I presented a paper looking to this end.[2] There are gentlemen here present who discussed that paper and who may perhaps recall the remarkable unanimity between the teachers of mathematics and those of engineering as to the results most to be desired in teaching mathematics to engineering students, and, indeed, as to the best available methods for producing such results. This movement is old. Most of the ideas which have been brought out here were first conceived a long time since. Nevertheless, it is good to get together and talk them over, and such discussions may result in help to the individual teacher.

We have heard here much of the ideal which the engineering school should set before itself, but it might well be asked what problem is presented first to the school as a matter of fact? President Woodward put it in part when he spoke of the difficulty of getting the right men in the schools when operators are so eager for good men and are competing on the basis of "so much per month." And what do the employers demand? They call for men who can do something, men who can think in a logical and common-sense way, but, withal, when they leave the school can be put to some immediate use. The first problem confronting the engineering college is how to meet this demand, for the demand must be met in some degree at least or the college will cease to train men.

It is inevitable that the character of this demand shall influence largely what the school must do. The call is not for men highly trained in mathematics, however much we may feel it ought to be. It is for men who know well a little mathematics, and who can do something with it, who can use it "as a tool." And, however obnoxious that expression may be to a mathematical teacher, he who forgets or disregards the fact which lies behind it will surely weaken his instruction of engineering students.

I do not defend the specification of the employer, I point to the fact with which we must deal. Personally I am inclined to find fault with it, but the matter rests largely in the hands of the practising engineer. He, though he often objects to the college product, is to a great extent responsible for its general make-up. In the long run and within reasonable limits he can have what he wants. Sometimes he is inclined to require too much technical knowledge on the part of the graduate. His brother teaching in the college in order to meet his requirement says to the teacher of mathematics I must have those students ready earlier with their mathematics. This fact, together with the general tendency in the colleges to raise the standards, causes

[2] See *Proceedings* of Society for Promotion of Engineering Education, Vol. V., 1897, p. 139.

the mathematical training to be crowded into the first year and a half or two years, when the student is least mature. More of it is being pushed back to the secondary school, and, in turn, into the grades. Mathematical concepts are difficult, and with President Woodward I am inclined to think we are demanding too much, and calling for it too soon. Covering less ground and at a slower pace will help to make better engineers.

The student comes to the engineering school with the notion that he is to be filled up with a lot of technical knowledge, the items of which will be used by him when he is a practising engineer. He seems unable to comprehend that he is in college to acquire mastery over his own powers. He is eager for useful facts and of course he forgets most of those he learns not a great while after leaving college. The forgetting is to be assumed. Under such conditions the task before the teacher of mathematics, and quite as well before the teacher of engineering, is to do his utmost to train his student to think logically and accurately about things. To this end there seems to me nothing so efficient as the solution of a large number of carefully chosen problems. Indeed what is one's life, if it be active, except meeting a never ending succession of problems which must be solved if success is to be gained? If you can teach your student to take vigorous hold of a problem, to first assemble all the facts which bear on the question, then from the facts to reason logically to a sound and safe conclusion, you have started him well whether his aim be engineering or otherwise.

Of transcendent importance is the teacher, his personality, his attitude toward his work, his knowledge of his students, not as a class, but of each as a human being. If we can procure the teacher who can idealize his work, who can show sustained enthusiasm for it and perform cheerfully the drudgery we heard mentioned a few minutes ago, we can safely leave detailed methods to him. Whatever methods such a man adopts in the classroom are likely to be effective.

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Michigan College of Mines

THE BRITISH MUSEUM OF NATURAL HISTORY

On July 28 a deputation, which included Mr. F. Darwin (Cambridge), Professor Cossar Ewart (Edinburgh), Professor Sedgwick (Cambridge), Dr. Marr (Cambridge), Professor Hickson (Manchester), Professor Bourne (Oxford) and Professor Graham Kerr (Glasgow), waited on the Prime Minister (Rt. Hon. H. H. Asquith, K.C., M.P.) in support of a petition sent to the late Prime Minister last autumn requesting that advantage should be taken of the present vacancy in the directorship of the Natural History Museum to hold an inquiry into the methods by which the museum is governed. The deputation was introduced by Sir W. Anson, M.P., Mr. Rawlinson, M.P., and Sir H. Craik, M.P.

According to the account in *Nature*, Professor Sedgwick said that zoologists thought it desirable to at once call the attention of the government to the desirability of instituting an inquiry into the methods of administration of the Natural History Museum, and that, if necessary, a widely signed memorial could be sent later on. In concluding a very full statement, Professor Sedgwick said:

> We are here to ask for a full official inquiry into the organization and administration of the Natural History Museum with a view to a reasonable treatment of the matter in the immediate future by his majesty's government.

Mr. Francis Darwin especially referred to the subordination of Cromwell Road to Bloomsbury. He said:

> Quite apart from the welfare of the Natural History Museum, it seems unfair to expect of the principal librarian that he should be responsible for Cromwell Road in addition to his other heavy

responsibilities. Nor can it be to the advantage of the British Museum that its principal officer should be so occupied. But it is when we look at the other side of the question that the faultiness of the arrangement becomes fully obvious. To choose a man distinguished for his technical knowledge and then to fail to give him reasonable freedom in the employment of his training and experience seems as bad a plan as it is possible to conceive. . . . I believe I am right in saying that when the late director was appointed his freedom was curtailed. It was, I think, unavoidable that in these circumstances difficulties should arise, and I feel very strongly that we ought to make the recurrence of such difficulties impossible; and this can only be done with certainty by making the Natural History Museum an independent unit.

This view was supported by Professor Bourne, who stated that

The Natural History Museum will not be placed upon a satisfactory footing until it is placed under the control of a body of trustees separate from that which is responsible for the control of the British Museum at Bloomsbury.

Professor Hickson pointed out that, notwithstanding the representations made by men of science during recent years,

No changes or reforms had been effected, and the administration is practically the same now as it was before the collections were removed from Bloomsbury, and that for seven months the museum has been deprived of the services of both a scientific director and a keeper of zoology.

Professor Ewart directed attention to the present unsatisfactory method of appointment of the director and of the subordinate members of the staff of the Natural History Museum; Professor Kerr said that, owing to the dissatisfaction which exists amongst men of science, it is "essential to hold a careful inquiry into the whole question of the organization and administration of the Natural History Museum before coming to a decision as to the remedial measures to be adopted," and Dr. Marr directed attention to the inadequate representation in the museum of those important branches of geology which are distinct from botany and zoology.

The Prime Minister, according to an official report which has been supplied, replied as follows:

He expressed his profound satisfaction at meeting so many eminent men of science. He pointed out that, as regards the administration of the museum, the trustees are a statutory body with whom the government were powerless to interfere. He confessed himself still unable to grasp in what way the museum failed to perform its functions. The arguments advanced by so many of the deputation as to the management by the trustees applied equally to the Bloomsbury museum. The trustees, men of wide experience and great distinction, were equally cognizant of natural history and archeology. He announced that the trustees were about to appoint a keeper of zoology, and that it was not intended to abolish the directorship, but only to wait to ascertain who was the best man for this responsible position. He sympathized with the view that the director should have a free hand in the management of his department, and promised to convey to his fellow-trustees of the British Museum all that the deputation had suggested.

LECTURES IN CONNECTION WITH THE INTERNATIONAL CONGRESS OF TUBERCULOSIS

IN connection with the congress, which meets in Washington from September 21 to October 12, a series of special lectures will be delivered in Washington and elsewhere by eminent foreigners. The names of the speakers and the cities in which they will lecture are as follows:

"Studies in Tuberculosis in Domestic Animals and what we may learn regarding Human Tuberculosis": Bernard Bang, of Copenhagen, at Washington, October 3.

"Les nouveaux procedes de diagnostic precoce de la Tuberculosis": A. Calmette, of Lille, France, at Philadelphia, September 26.

"La Lucha contra Tuberculosis en la Republica Argentina": Emil Coni, of Buenos Ayres, at Washington, October 2.

"The Causes which have led to the Past Decline in the Death Rate from Tuberculosis and the Light thrown by this History on Preventive Action for the Future": Arthur Newsholme, of Brighton, at Washington, September 29.

"Social Life and Tuberculosis": Gotthold Pannwitz, of Berlin, at Philadelphia, September 24.

"The Anti-tuberculosis Program—Coordination of Preventive Measures": R. W. Philip, of Edinburgh, at Boston, October 6.

C. H. Spronck, of Utrecht, at Boston, October 7.

"Tuberculosis of the Heart, Blood and Lymph Vessels": Andres Martinez Vargas, of Barcelona, at New York, October 9.

"The Evolution of the Treatment of Pulmonary Tuberculosis": Theodore Williams, of London, at Philadelphia, September 25.

"La Lutte Contra la tuberculose dans les grandes villes par l'Habitation; methodes scientifiques modernes pour sa Construction" (joint lecture): Dr. Maurice Letulle and M. Augustin Rey, at Washington, September 30.

Dr. L. Landouzy, of Paris, at Baltimore, October 5.

"Biology of the Bacillus": Dr. A. A. Wladimiroff, of St. Petersburg, at Washington, September 28.

"Collateral Tuberculosis Inflammation": Professor N. Ph. Tendeloo, of Leiden.

THE INTERNATIONAL FISHERIES CONGRESS

A FURTHER announcement of the congress, to be held from September 22 to 26, states that at 9:30 A.M. on September 22 the foreign delegates will be received by the secretary of state in the Diplomatic Reception Hall of the State Department. The initial meeting of the congress will be held at 10 o'clock in the hall of the National Geographic Society, where addresses of welcome on behalf of the United States will be delivered by Hon. Oscar S. Straus, secretary of commerce and labor, on behalf of the District of Columbia by Hon. Henry L. West, commissioner of the district, and on behalf of the American Fisheries Society by Dr. H. M. Smith, president of the society. The meeting of organization will be held in the banquet hall of the New Willard Hotel, Pennsylvania Avenue and Fourteenth Street, on the afternoon of September 22, at an hour to be announced. The regular sessions of the congress will be held daily, morning and afternoon, at times to be announced, at the New Willard Hotel. The president of the United States will receive the members of the congress at the White House. The secretary of commerce and labor will give an evening reception. Luncheons will be tendered by the American Fisheries Society, the Blue Ridge Rod and Gun Club and the Alaska Packers Association, respectively, and there will be a subscription banquet at which the official representatives of foreign governments will be the guests of the congress. Visits to places of interest and other entertainment have been arranged for by the local reception committee. Arrangements have been made to permit members who so desire to inspect the important fisheries of New England. An attractive itinerary has been arranged embracing the entire week following the sessions of the congress and including visits to New York City, Narragansett Bay, Woods Hole, Boston and Gloucester, at each of which places local committees and individual residents will provide demonstrations of fishery methods and incidental entertainment. The methods of oyster culture employed on the great New England beds, the pound-net fishery, the purse-seine fishery, inspection of fish markets and vessels, the methods of deep-sea research, and other matters relating to the fisheries will be shown. Special itineraries will be arranged for members who may desire to visit other fisheries and hatcheries, and letters of introduction will be furnished.

SCIENTIFIC NOTES AND NEWS

MR. F. J. SEAVER, assistant botanist of the North Dakota Agricultural College, has been appointed director of laboratories in the New York Botanical Garden.

DR. W. H. WELCH, of the Johns Hopkins Medical School, will deliver, on November 20, the principal address on the occasion of the dedication of the new building devoted to experimental medicine of the Medical College of Western Reserve University.

SIR GEORGE HOWARD DARWIN, professor of astronomy at Cambridge, has been elected a corresponding member of the Berlin Academy of Sciences.

THE Royal Astronomical Society, London, has elected corresponding members as follows: Dr. E. B. Frost, director of the Yerkes Observatory; J. G. Hagen, S.J., director of the Vatican Observatory; M. Benjamin Baillaud, director of the Paris Observatory; C. L. W. Charlier, director of the observatory at Lund,

and Johannes Franz Hartmann, of the Astrophysical Observatory at Potsdam.

THE Alvarenga prize of the College of Physicians, Philadelphia, for 1908 has been awarded to Dr. William T. Shoemaker for an essay entitled "Retinitis Pigmentosa."

THE Denny gold medal, provided for by the late Peter Denny, LL.D., and granted each session for the best paper read before the Institute of Marine Engineers, London, has been awarded to Mr. Robert Elliott, B.Sc., for his paper on "Repairs to Ships," read during the session 1907–8.

DR. WM. T. GLAZEBROOK is acting president of the second Optical Convention, which will meet in London in May of next year.

DR. KURT WEGENER has been appointed director of the Observatory of Samoa.

DR. FRANZ LINCKE, of Göttingen, has been appointed director of the aeronautic department of the Physical Society at Frankfort.

PROFESSOR SIMON NEWCOMB, U. S. N. (retired), was received in audience by Emperor William, on August 17, at Wilhelmshohe, after which he was invited to luncheon. Professor Newcomb thanked the emperor for the order Pour le Mérite for Science and Arts bestowed upon him three years ago.

DR. ADOLF WÜLLNER, professor of physics in the Aachen Technical Institute, has celebrated the fiftieth anniversary of his doctorate.

DR. N. L. BRITTON, director of the New York Botanical Garden, and Mrs. Britton left New York for Jamaica on August 22, expecting to return at the end of September.

DR. WILLIAM W. KEEN, who has been abroad for more than a year and a half, has returned to Philadelphia.

THE president of the British Board of Trade has appointed Lord Rayleigh, O.M., Professor J. J. Thomson, F.R.S., Dr. R. T. Glazebrook, F.R.S., Sir John Gavey, C.B., and Mr. A. P. Trotter, to be the British delegates to the International Conference on Electrical Units, which is to assemble in London on October 12. Mr. W. Duddell, F.R.S., and Mr. M. J. Collins, of the Board of Trade, will act as secretaries to the British delegates.

KING EDWARD has made the following appointments: David W. Finlay, M.D., F.R.C.P., London, professor of the practise of medicine in the University of Aberdeen, to be one of the honorary physicians to the king in Scotland, in the room of Sir Thomas McCall Anderson, M.D., deceased. Sir William Macewen, F.R.S., M.D., regius professor of surgery in the University of Glasgow, to be one of the honorary surgeons in Scotland, in the room of Sir Patrick Heron Watson, M.D., deceased. James Little, M.D., regius professor of physic in the University of Dublin, to be one of the honorary physicians in Ireland, in the room of Sir John Thomas Banks, K.C.B., M.D., deceased.

DR. FRIEDRICH PAULSEN, professor of philosophy, at Berlin, died on August 14, from cancer, aged sixty-two years.

THE death is also announced of Dr. K. Zoeppritz, observer in the Geophysical Institute at Göttingen, and known for his work on atmospheric electricity.

THE ninth International Congress of Geography was held in Geneva from July 27 to August 6, and was preceded and followed by interesting excursions conducted by local geographers. The occasion was also notable as the jubilee celebration of the founding of the vigorous Geographical Society of Geneva. The division of the assembly into numerous sections, including a number of new divisions of science not admitted to previous geographical congresses, much increased the interest in the papers, though it disposed of the program long before the congress adjourned. American geographers were represented by Professors Davis and Johnson, of Harvard; Brigham, of Colgate; Cleveland, of Williams; Fenneman, of Cincinnati; Leverett, Hobbs and Scott, of Michigan; and Professor Simon Newcomb and Dr. D. T. Day, of Washington.

THE French Association for the Advancement of Science, which has been meeting at Clermont-Ferrand under the presidency of M. Paul Appell, professor of mathematics at the Sorbonne, adjourned to meet next year at Lille. The meeting of 1910 will be held at Toulouse. One of the addresses at the recent

meeting was by Sir William Ramsay on the results of his researches into radioactive substances.

At the meeting of the Paris Academy of Sciences on August 10, Mme. Curie stated that working in collaboration with Mlle. Gleditch, she had been unable to confirm Sir William Ramsay's experiment, by which copper appeared to be transmuted into lithium by radium emanations. With copper receptacles containing distilled water absolutely without any trace of lithium certain traces of that metal made their appearance after 24 hours under the application of radium. The same result took place in the case of a quartz receptacle. Mme. Curie and Mlle. Gleditch thereupon used a platinum apparatus. They placed in it distilled water and used copper salts produced in the laboratory entirely free from lithium. These copper salts were then exposed to emanations of radium, and no trace of lithium was discovered.

Among guests from abroad who attended the recent Sheffield meeting of the British Medical Association were Professor Axenfeld (Freiburg), Professor Bouchard (Paris), Dr. Bossi (Genoa), Dr. Depage (Brussels), Professor Fuchs (Vienna), Dr. A. M. Gilchrist (Nice), Professor Axel Holst (Christiania), Dr. Jacoby (Brussels), Professor Kolli (Bern), Professor Von Kronig (Freiburg), Dr. Just Lucas-Championnière (Paris), Professor Luigi Mangiagalli (Milan), Professor Alb. Neisser (Breslau), Dr. Noiré (Paris), Professor Onodi (Buda Pesth), Dr. Sabouraud (Paris), Professor Zweifel (Leipzig), Professor Tillmanns (Leipzig), Dr. C. Willems (Ghent), Professor Ambrose Monprofit (Angers), Professor Alb. Plehn (Berlin), Dr. Marc Armand Ruffer (Alexandria), Dr. J. G. Willmore (Alexandria), Dr. E. Marchoux (Paris), Professor Pozzi (Paris), Professor Gilchrist (Baltimore), Professor Garceau (San Francisco), Professor Holt (New York), Dr. Charles Leonard (Philadelphia), Dr. J. B. Murphy (Chicago), Dr. H. H. Pratt (Baltimore), Dr. Marcy Riverton (New Jersey), Dr. Maurice Richardson (Boston).

At the International Zoological Congress, to be held at Graz in 1910, the subject of the Emperor Alexander III. prize is "The Intervention of Mechanical Phenomena in the Transformation of Animal Forms," and the subject of the Emperor Nicholas II. prize, "A Monographic Study of the Group of Plathelminthes." The essays must be sent in by June 1, 1910, to the general secretary, M. Blanchard, 226 Boulevard St. Germain, Paris.

We have received from Dr. F. A. Bather an announcement of a double index to the generic and specific names in E. Desor's "Synopsis des Echinides Fossiles," preceded by a "Note sur les Dates de Publication," by M. Jules Lambert. Further information may be obtained from Dr. Bather at the Natural History Museum, South Kensington, S.W.

Consul General Richard Guenther, of Frankfort, notes that at the annual meeting of the Association of German Engineers lately held at Dresden, announcement was made that the great work of compiling and publishing the new technical dictionary, which was conducted under the auspices of the association, had to be stopped because it was found that the expenses would amount to more than four times the estimates. Mr. Guenther adds: "The great progress in science and industries had created a vast mass of new terms and matter largely in excess of what had been estimated at the beginning. This stoppage is to be greatly regretted, as the want of a new technical dictionary and encyclopedia is acutely felt by thousands of persons engaged in scientific research, in all lines of commerce and production, in literature, journalism, and in the administration of state and municipal government. It is, however, satisfactory to note that the executive board of the Association of German Engineers has made strenuous efforts to take up and complete this valuable work, and has succeeded in obtaining therefor the aid of the federal government of Germany and of the ministry of education of the Prussian kingdom."

We learn from *Terrestrial Magnetism and Atmospherical Electricity* that arrangements

are being made to secure in cooperation with the Canadian government, magnetic observations on the eighteen-months' cruise of the Canadian steamer *Arctic* (formerly the *Gauss*), among the Arctic Islands north of the Magnetic North Pole. Mr. W. E. W. Jackson has been detailed by the minister of marine and fisheries, to duty on the *Arctic*. Magnetic, meteorological, electric and tidal observations will be attempted. Dr. Bauer recently visited the *Arctic* at Quebec, commanded by Captain J. E. Bernier, and arranged with him and Professor Stupart at Toronto regarding the magnetic and electric instruments and accessories to be supplied by the Department of Terrestrial Magnetism and as to the methods of work to be followed.

The London *Times* states that Mr. C. Kenrick Gibbons has presented to the Zoological Gardens a large number of the small fresh-water fish from Barbados known as "millions" (*Girardinus poecilloides*). These little fish, which have been placed in a tank in the tortoise house, are of special interest because of their supposed action in preventing malaria. Malaria is very much less common in Barbados than in other West Indian islands, and it has been suggested that this freedom is due to the presence of enormous quantities of the "millions" in the fresh-water pools. The little fish are very voracious, and destroy large numbers of the larvæ of mosquitoes that spread malaria. The males are about half an inch long, with brilliant iridescent colors, and large black spots on the sides. The females are considerably larger and less highly colored. It is understood that experiments are going to be made with the introduction of these fish into tropical countries where malaria is prevalent.

There is, it appears, in Great Britain a National Canine Defense League, which claims that 1,250 medical men have signed a petition in favor of a bill now before Parliament exempting dogs from vivisection, and further that 388 members have given their written promise to support the measure.

UNIVERSITY AND EDUCATIONAL NEWS

The College of Agriculture and Mechanic Arts, of Hawaii, was established by act of the legislature last March. The new college will be located at Honolulu. A site for the campus and buildings has been secured in Manoa Valley, commanding a fine view of the ocean. Professor John W. Gilmore, of the Pennsylvania State College, has been chosen president. The college will open on September 4.

On August 15, fire destroyed the main building of the large barn of the Massachusetts Agricultural College, at Amherst. Two valuable Holstein and Jersey bulls and eleven calves, together with a large quantity of hay and farm machinery, were also burned. The loss is estimated at about $40,000.

Two upper floors of Curtis Hall, used as dormitories, at Tufts College, were destroyed by a fire of unknown origin on the 16th instant, with damage of $5,000.

Professor Otto Frank, of Giessen, has been elected professor of physiology, at Munich, to succeed the late Professor Carl von Voit.

Dr. Nagel, of Berlin, has become professor of physiology at Rostock.

Dr. Curt Hensel, professor of mathematics at Marburg, has been called to Leipzig.

THE HARVARD BUSINESS SCHOOL

The Official Register of Harvard University in its issue of this week contains the first detailed announcement of the Graduate School of Business Administration which will be opened to students on October 1, under the direction of Dean Edwin F. Gay. The unique feature of the school, both in Harvard experience and in the educational world, is that the new school starts with the requirement of a college degree for admission. Upon that foundation of liberal education it rests a severe two years' course, partly prescribed and partly elective, leading to the degree of Master in Business Administration and representing work in the following special fields: Banking and finance, accounting and auditing, insur-

ance, industrial organization, transportation, commercial law, economic resources, and public service. Courses in French, German, and Spanish Correspondence will be offered with the special object of enabling graduates of the school to read and write letters in these languages and to understand the accepted forms of business correspondence. Two of the most important courses to be offered will be entitled respectively: "Corporation Finance" and "Industrial Organization." Among those who have been engaged to lecture on Corporation Finance are Herbert Knox Smith, Commissioner of Corporations in the U. S. Department of Commerce and Labor; Frederick P. Fish; Professor Edwin S. Meade, of the University of Pennsylvania; James F. Jackson, ex-chairman of the Massachusetts Railroad Commission; C. C. Burlingham, of New York, receiver of the Westinghouse Company; Judge C. M. Hough, of the U. S. District Court for the Southern District of New York; F. A. Cleveland, of the New York Bureau of Municipal Research, and G. W. Wickersham, the New York lawyer. Among those who have been engaged to lecture on Industrial Organization are Frederick W. Taylor, ex-president of the American Society of Mechanical Engineers, and a leading authority on factory organization; J. O. Fagan, a signalman employed by the Boston and Maine Railroad, the author of the recent articles in the *Atlantic Monthly* entitled "Confessions of a Signalman," and Russell Robb, of the firm of Stone & Webster, Boston.

One of the most important features of the school will be the practical work required of each student in the summer. The object of this work will be twofold, first, to teach the student from practical experience and observation the elements of business that can not be taught in the class-room, and, secondly, to bring them in contact with the men with whom their life work is to be done. The school does not pretend to graduate men who will begin at the top or high up in their several lines of business. It does aim to teach them how to work and how to apply powers of observation, analysis, and invention to practical business problems.

DISCUSSION AND CORRESPONDENCE

CONCERNING TWO DEFECTIVES

To the Editor of Science: Inquiries from various parts of this country show that the newspapers have given wide publication to a yellow telegram from San José concerning the Lick Observatory. It was reported that the observatory carpenter, going violently insane, had driven the astronomers and a party of visiting students out of the buildings, that the telescopes were at the mercy of his wrath, and that he was overcome and put under restraint at the expense of a struggle. The facts are that the carpenter became mildly insane; that no one left the buildings on his account; that he was watched and could have been apprehended at any time; that he was not near the telescopes; and that he submitted meekly to arrest by the sheriff. A competent jury would probably decide that this mild lunatic was less harmful to the public than the penny-a-liner who took advantage of millions of helpless newspaper readers. Is the Associated Press at the command of such as he?

W. W. Campbell

SORES ON COLTS

To the Editor of Science: Some ten or twelve years ago I had about fifty colts born on my farm. When they were foaled, they appeared without a blemish. But within ten days after, the hair would fall off a spot averaging two inches long and a half inch wide, leaving a raw sore, which would, in the course of ten days, heal over, leaving a scar. Shortly after, a new crop of hair covered the spot, which by its different "sheen" would render the location of the "sore" visible for several months. The location of this sore is invariably in the hollow of the hock joint, upon the external facies of the leg, with the long diameter perpendicular as the colt stands, thus being somewhat diagonal to the Tendo Achilles. Fifteen years of close observation shows it to be an invariable feature of a colt's life in Louisiana. A number of years ago I called the attention of Dr. W. H. Dalrymple, of Baton Rouge, La. (who needs no introduction

from me), to the sore; and he informs me that by his subsequent observations it seems to be universal at least in America. Asiatic horses not yet having been observed in this respect. I feel sure it is a feature of a horse's life universally.

Many times I have amused myself by telling the owner of a colt, when I had informed myself of its age, that "your colt has a sore on each of its hinder legs."

"When did you see it?" replies the owner.

On my rejoinder that "I have never even seen the colt," he would naturally "say remarks."

The attention of biologists is called to this fact, and *theories* requested—as the writer has none.

L. S. FRIERSON

SCIENTIFIC BOOKS

The Animal Mind. By MARGARET FLOY WASHBURN. New York, The Macmillan Co. 1907. Pp. x + 333.

In this book the author has brought together a wide series of facts which represent the main results achieved in the field of animal behavior during the last few years. It is designed both as a text-book in comparative psychology and as a ready and a convenient reference book. The volume will be of untold value to the general scientific reader, and to the comparative psychologist who has confined himself somewhat narrowly to a particular phase of animal behavior.

The material gathered together in this volume has been arranged in a logical and systematic way. The book affords, consequently, easy orientation into any given phase of the field. The style of presentation is clear and readable. It is the hope of the reviewer that this volume may fall into the hands of the general reader and thereby serve as a counter-irritant to a number of books which deal presumably with the "truth about animals." Certainly any one who has had the benefit of ordinary college training can read the book with profit.

Miss Washburn's opening chapters deal intelligently with the difficulties in the way of observing the reactions of animals; with the methods of observing such reactions; with the methods of interpreting observed facts; and with the evidence for the presence of mind in animals as inferred, on the one hand, from structure and, on the other, from behavior.

In the chapter on the mind of the simplest organisms the author treats first of the structure of the lowest organisms, next of the observed facts about their behavior, and then attempts to construct from these data the kind of mind such organisms must have—if they are conscious. This attempted construction of the mind of lower animals is a somewhat forlorn and hopeless task. The necessity of such a task is felt mainly by those psychologists who think of mind largely in terms of structure.

The chapters dealing with the sensory discriminations in animals are especially well done. Under the heading of Sensory Discrimination: The Chemical Sense, Miss Washburn brings together a vast amount of material taken from the experiments made upon animals ranging from the cœlenterates to the vertebrates. The many research articles dealing with this subject are scattered and inaccessible. The author has done a real service in bringing them together and giving them systematic treatment.

In the chapter on hearing the author, while giving a good résumé of the field, makes the mistake of saying that birds have no cochlea. I quote her in detail as follows (p. 119):

> The cochlea is supposed to be the portion of the human ear upon which the power to distinguish pitch differences rest. *Yet birds have no cochlea* [italics mine], though if we grant that animals which produce sound are those which are able to hear them, some birds at least must be capable of pitch discriminations of wide range and great acuteness. The powers of imitation so often evidenced in bird song are proof that this is the case.

Edinger's statement concerning the cochlea in birds is as follows:

> The cochlea is only slightly developed in fishes, but in birds it reaches a fair development.[1]

Wiedersheim has the following to say concerning the cochlea of birds and reptiles:

[1] "Anatomy of the Central Nervous System, etc.," Hall's English translation, 5th edition, p. 91.

Bei den ersteren wächst die Schnecke immer weiter canal-artig aus (Ductus cochlearis) und erfährt schliesslich bei Crocodiliern und Vögeln eine Krümmung sowie eine schwache Spiraldrehung. Hand in Hand damit geht eine immer schärfere Differenzierung der Lamina (Membrana) basilaris und der Papilla acustica basilaris. Beide strecken sich mehr und mehr in die Länge, und zugleich ist eine Scala tympani und vestibuli schon deutlich angelegt.[2]

It is barely possible that the author had in mind the lack of the arches of Corti in the auditory apparatus of birds. This is admitted by comparative neurologists;[3] but a well-marked basilar membrane is at hand. It will be remembered that one consideration which led Helmholtz to abandon the notion that the arches of Corti alone are responsible for the sensing of the differences in pitch and to assign that function to the fibers of the basilar membrane was due to the fact that birds possess the latter structure but not the former.

The author treats of Spatially determined Reactions and Space Perceptions, in two chapters. She discusses here: reaction to a single localized stimulus; orienting reactions; reaction to a moving stimulus; reaction to a retinal image; reactions adapted to the distance of objects. The various reactions considered in this part of the book should in all probability be treated together, but it is somewhat a stretch of the imagination to deal with them under a title so suggestive of organized mental life. Aside from this point we must comment upon the value of the organization of this complex material. Those of us who have followed in some measure the advances made in the study of the lower organisms know what a tremendous task it must have been to go through this field and to gather up the important facts and then systematically to organize them into a readable whole.

The latter part of the book deals with the modification of conscious processes by individual experience; the memory idea; and some aspects of attention.

The book as a whole is so well done that we venture the opinion that its usefulness will continue for several years to come. Its arrangement is such that the results of later researches as they appear from time to time may be easily incorporated into successive editions.

JOHN B. WATSON

THE UNIVERSITY OF CHICAGO

[2] "Vergleichende Anatomie der Wirbelthiere," funfte Auflage, p. 324. Cf. also the monumental work of Retzius on the auditory organs of vertebrates.

[3] C. Hasse. See Helmholtz's "Sensations of Tone," p. 146.

A Pocket Handbook of Minerals, designed for use in the field or class-room with little reference to chemical tests. By G. MONTAGUE BUTLER, E.M., Assistant Professor of Geology and Mineralogy, Colorado School of Mines, Golden, Colorado, United States Deputy Mineral Surveyor. 16mo, pp. ix + 298. 89 figures. Morocco, $3 (12/6 net). New York, John Wiley & Sons; London, Chapman & Hall, Limited. 1908.

This book is designed for both field and class work and to fill a space between works "too cumbersome" for the field and works "so condensed as to confuse rather than aid."

Two hundred species are described in terms of those characters which the author considers best help in their determination, and preference is given to the so-called "physical features." Each species is described in the same fixed order and certain chosen characters are brought into especial prominence by the use of heavy-face type so that "a mere glance at a page will often suffice to recall the appearance of a mineral."

In the selection of important characters as indicated by heavy-face type, very great prominence is given to cleavage and very little to blowpipe or acid tests. It is certainly to be questioned whether in the field with average specimens, not usually crystals, even the trained mineralogist can determine more than the existence or non-existence of marked cleavage and in certain instances the approximate cleavage angles. The blowpipe is usually as available as the goniometer or microscope.

Following the descriptions of species, which occupy 270 pages, are some ten pages of miscellaneous tables including lists of commercially

important ores, retail prices of cut gems, values of metals and minerals; then follows an admirable glossary in which, however, some of the fundamental terms, such as crystal, mineral and polarize, are not defined with scientific accuracy.

The tables which follow the index are summaries of the descriptions, characters in parallel columns and minerals in order of description.

The book is of convenient size for the pocket and embodies much easily accessible and useful information. In spite, however, of the fact that it is, as explicitly stated, designed for the determination of minerals, its value in the absence of all systematic schemes would seem to be rather to refresh the user's memory as to the characters of known or suspected minerals, than as a guide to the determination of unrecognized material.

A. J. Moses

Analysis of Mixed Paints, Color Pigments and Varnishes. By C. D. Holley, Ph.D., and E. F. Ladd, B.S., Professors of Chemistry, North Dakota Agricultural College. New York, John Wiley & Sons. Pp. 235.

This book presents in a more accessible and considerably enlarged form the results of the work done in connection with the enforcement of the North Dakota paint law. It gives the latest and best methods for the analysis of the substances mentioned in its title, and, what is still more valuable, the composition of these articles as found on the American market.

The method for the analysis of linseed oil, however, is incomplete, no mention being made of the process for detecting fish oil in it with certainty.

Incidentally it furnishes a striking commentary on the honesty and integrity of the American paint and oil trade. The authors' investigations showed "white leads" which contained no lead carbonate and but five per cent. of lead sulphate; other pigments were found which were branded in a manner calculated to mislead. Not content with this sort of fraud, water, in some cases to the extent of twenty-five per cent., was mixed with the paints and these put up in packages which were 10 to 13 per cent. short in weight or measure! The authors have done a real service in showing up such conditions.

The work is one of the best contributions to the literature of these subjects that have appeared, dealing not only with analyses, but also with specifications, and the application and testing of paints on a large scale, and should be in the library of every one having to do with the subjects treated.

A. H. Gill

SPECIAL ARTICLES

SOME CONDITIONS AFFECTING VOLCANIC ERUPTIONS

In the study of such natural phenomena as are difficult to investigate by reason of inaccessibility, or of danger to the observer, it is natural and often advantageous to consider some analogous, but less obscure phenomenon and, from a careful study of this, to deduce the laws which govern the former. A case in point is that of a volcano in eruption which, by its very nature, prohibits close inspection, but with which a certain degree of parallelism is found in the action of geysers. More than thirty years ago Fuchs called attention to the similarity existing between the two, comparing the column of water in the geyser tube to the lava in the interior of a volcano and stating that geysers "ont encore une grande importance en ce sens qu'ils nous permettent de nous faire une idée claire des phénomènes qui produisent les éruptions volcaniques." (K. Fuchs, "Les volcans et les tremblements de terre.") In the light of modern volcanological science, however, this generalization of the term "éruptions volcaniques" will be found too sweeping, for it is clear that the action of a trachytic volcano, whose highly silicious magma is at best in a viscous state, can with difficulty be considered as analogous to that of a geyser where fluidity is the most evident characteristic. A comparative study of the two phenomena should, therefore, be prefaced by the explicit statement that the volcano in consideration is of the basaltic type, with lava which is liquid at the temperature of action, and con-

sequently subject to the laws of hydrostatics. With this understanding let us examine for a moment the points of analogy and of difference between the two.

In the lower portions of the geyser-tube the water becomes heated by conduction above 100° C. Ebullition can not take place because of the pressure of the water above, and the excess of heat represents stored energy—a latent force which will manifest itself upon relief of the pressure due to the above standing water column. This latter may be considered as divided into an indefinite number of zones each having a critical temperature depending on its position, that of the surface layer being 100° C. The column of water is progressively heated from below by conduction and convection until the water of some zone attains its critical temperature; boiling takes place, relieving the pressure on the water just below, which, in its turn, bursts into ebullition, and thus a progressive reactionary movement is set up with a rapidly increasing amplitude of vibration until most of the energy latent in the superheated depths is set free, completing the eruption. The action is often begun by a raising of the water, which, at some zone, is near its critical temperature, into a position of lesser pressure, when boiling will begin and the reactionary process be initiated—in either case it will be noted that it is the rapid diminution of pressure by the act of ebullition which institutes the vibratory process. The reader will here recall that, in the bursting of steam boilers, the action is also thought to be multiple, the too rapid escape of steam from a broken part resulting in the sudden liberation of energy latent in the superheated water, thus completing the explosion.

Let us now consider the action of a basaltic volcano, assuming the central conduit to be filled with liquid magma up to the crater. The lava in the conduit below the crater will be subjected to a pressure increasing proportionately with the depth, and the water and other gases occluded in the magmatic material will, under such conditions of pressure and temperature, be possessed of an enormous latent force of expansion. An upforcing of the lava column or a rapid increase of temperature may, therefore, precipitate an eruption by instituting a reactionary process of gaseous expansion exactly as in the case of the geyser. The greater dimensions of the volcano, together with the density of the magma, will render this reactionary process more gradual than in the geyser; inertia and momentum will prolong the vibratory periods, and days instead of minutes may be required to bring about the culmination. It may, indeed, be questioned if, in many cases, an elevation of the lava column, or an increase in its temperature would be sufficiently sudden to initiate the reactionary process, but this may be brought about in another way. An interesting point of divergence from the geyser lies in the height of the volcanic cone within which the lava may rise to a considerable elevation above the earth's surface. Pressure of the lava column on the walls of the cone aided by explosions from below and the re-fusing power of the magma may fissure the cone and permit of a lateral outflow. If this is sufficiently rapid to considerably reduce the level of the lava, the pressure on the magma below is greatly diminished and gaseous expansion takes place, an immense amount of vapor is set free to do battle with the solid materials (due to collapse consequent to the withdrawal of a large quantity of lava), and a great eruption is thus produced. In my opinion, we need not conclude that the rapid gaseous expansion extends to the greater depth of the volcanic conduit and much less to the fire-pocket itself, where the magma, by reason of pressure, may be in a pasty or quasi-solid condition, but the active expansion would be limited to a zone whose depth will bear a certain relation to the original height of the lava column and the difference of level resulting from the outflow. The greater the difference of level the deeper will be the zone of active expansion and the sum total of energy released. The more *rapidly* the disleveling is produced, the more violent will be the explosive effects, although the total

quantity of vapor expanded may be independent of this rapidity, *providing always that the lateral drainage is sufficiently rapid to produce the difference of level.* For it is precisely this which makes the difference between a catastrophal eruption of this class and one which is merely a phase in the progression of an eruptive period. Terminal or subterminal lava streams, or the sluggish forms of lateral outflows can not produce a material reduction in the level of the lava—the former by reason of their location and the latter because the slow drainage is continually compensated by alimentation from below; there being no rapid diminution of pressure, there is consequently no abnormal expansion.

In considering further the points of divergence between geyser and volcano it would seem that, aside from the obvious dynamic and caloric disproportion—that is, the relative insignificance of the former in size, mechanical power and heat energy—a fundamental difference lies in the relative proportions of water and temperature. In the geyser the heat is moderate while water is abundant and, after an eruption, may freely flow into the central conduit, which it occupies in mass. But in the case of the volcano we may imagine that the water can reach the fire-pocket only by capillary infiltration and under such conditions of temperature and pressure as will cause its intimate union—possibly through complete dissociation—with the heated materials with which it comes in contact, forming thus an incandescent, eruptive magma. And this will be the case whether the temperature results from chemical combination of the water with oxidizable material (Davy), from mechanical friction and compression (Mallet) or from the retained original heat of the earth. The magma will augment in quantity, in temperature and in expansive power with the progressive infiltration of water, and, with its occluded gases will seek a vent at the earth's surface. If there exists, instead, a universal magma with already occluded water gases there will still be the same proportion of water to temperature.

But what is perhaps the most important point of difference between geyser and volcano—and to this all the preceding forms but an introduction—is that in the former, actual eruption is determined and brought about by conditions existing within the geyser itself, while in the latter this is not the case. The geyser is truly automatic, the volcano is not. In studying the development of volcanic eruptions one is irresistibly led to the consideration of modifying and controlling forces acting from without and which may even be extra-terrestrial. It is not denied that, given the progressive delivery of active volcanic material from below, an eruption would in time occur even without external influence, but it is claimed that, under actual conditions, eruption will inevitably be precipitated before such a time. If this is true, the study of such modifying influences becomes of the greatest importance, especially in connection with the foretelling of eruptions, and it is only from a profound conviction of its usefulness that the writer ventures to bring forward at this time an old and abandoned hypothesis—that of the luni-solar influence. I believe that the discredit into which this theory has fallen since the days of Palmieri is due partly to a not unnatural reaction from his somewhat extreme views on the subject and partly to a misunderstanding of the mode of action, the few attempts which have been made to show a correspondence between the lunar phases and volcanic phenomena being rather inconclusive. Riccò, in an interesting pamphlet,[1] gives tables showing, in four cases, a coincidence between luni-solar combinations and the eruptions of Stromboli, but neither the coincidence here nor the lack of it in five remaining cases seems very definite, because the time of the eruptions is merely given as the "date of the beginning of the periods of singular activity." External influence upon earthquakes has been more generally studied, Schmidt having presented[2] a carefully prepared summary of the effect of lunar distance, luni-solar positions,

[1] "Sulla influenza luni-solare nelle eruzioni, del Prof. Riccò."

[2] "Stud. üb. Vulk. u. Erdbeben," Leipzig, 1881.

barometric pressure, time of day, time of year and weather upon earthquakes, but each of these is considered separately, while it is only by combination that they are rendered effective. Falb, in a very studious work rarely quoted,[1] considers the luni-solar combinations with conclusions favorable to their coincidence with seismic movements, but the aggregate conclusions of all who have examined the subject do not form a definite and harmonious result. I believe this to be due in part to the very elaborateness of the methods used in treating an essentially simple subject and to taking into consideration a great number of very slight earthquakes and eruptive phases whose entry into the calculation has led to erroneous conclusions—it is like including the mortality of infants in computing the length of human life. I propose, therefore, in this paper, to make a preliminary examination of the subject as it has developed under my own studies during the past two years.

It will be impossible, however, to enter at this time into an exhaustive study of all the possible manifestations or transformations of energy by which the luni-solar influence may affect terrestrial volcanism—tidal action, atmospheric electric potential, electro-magnetic telluric currents, etc.—but, without excluding these, we may for the present simplify our conception of the influence by considering it as productive of a *gravitational disturbance of the terrestrial mosaic.* Imagine such a body as the earth subjected to the varying attraction of the sun and moon, now on one side of both, now revolving between the two like the armature of a giant dynamo in its field of magnetic force; with the moon in such close proximity as to exert a considerable *difference* in attractive force at the earth's center and at the nearest and farthest peripheral points and with an orbit so elliptical as to vary its distance by a factor of more than one tenth. And then consider that in the crust of this revolving earth sphere there exist volcanoes, which are at times in a condition of potential eruption and rock strata in a condition of stress amounting to potential faulting, and it will readily be seen how small is the power required to touch off these little accumulations of latent energy in proportion to the enormous forces involved. Note that the celestial influence is here considered as being exerted merely in the releasing of stored energy and not as being directly concerned in the accumulation of the latent forces. Note also that we have now cast off the restriction imposed earlier in this paper as to the type of volcano to be considered, all types being affected by the external influence.

Let us now consider in detail that which we have defined as "gravitational disturbance." When the sun and moon are in line with the earth, their combined attraction tends to deform the earth sphere to an ellipse—the tendency is resisted and a condition of stress results. When in quadrature their influence is largely neutralized and these changes, in connection with the earth's diurnal rotation, may be considered as the basis of the gravitational disturbance.

The greatest luni-solar gravitational effect, for these latitudes, will be produced when the sun and moon are at opposite ends of a diameter through the earth's center (opposition) and having a north and south declination respectively of 23° (solstice) and with the moon in perigee. These positions would also tend to produce the greatest ocean tides, but we should avoid considering this influence on the basis of tidal action—it is because of this, in my opinion, that much misunderstanding has arisen. We have not to do with a liquid ocean, where, under luni-solar attraction, a moving wave-form would produce, at the times of maximum effect, very high and very low tides, but we must consider a mosaic globe composed for the most part of solid materials in which the effects, although not of such amplitude as to be visible, will be more powerful and less ephemeral. We may imagine the mean luni-solar effect upon the earth during the several days of each favorable position to be a tendency to positive deformation of the sphere. This will result in a diminution of lateral pressures due to terrestrial gravitation

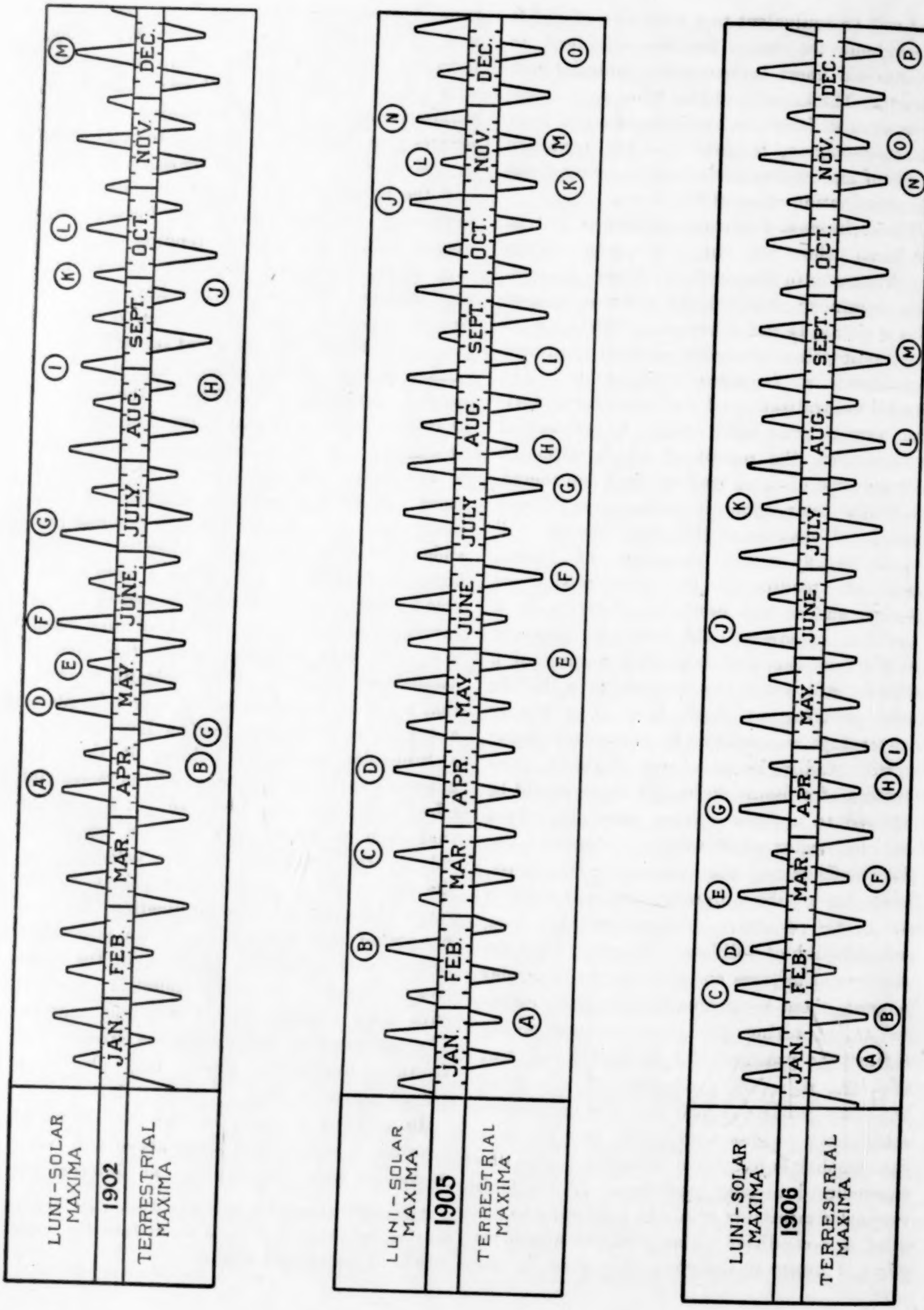
[1] "Grundzüge zu einer Teorie der Erdbeben und Vulkanausbrüche," Rudolf Falb, 1869.

and will be equivalent to a condition of uplift throughout the mass, fissures will tend to widen, and gases in magmatic material will expand. The extreme of this effect will, therefore, tend to favor the emission of gases from volcanic magma, to determine the explosive crises of an eruptive period and to bring about the culmination of great eruptions.

The *minimum* luni-solar influence will be produced when the sun and moon are in quadrature with the earth, with zero declination (equinox) and with the moon in apogee. But it should be noted here—and this is a most important point—that this celestial minimum constitutes a *terrestrial maximum.* The virtual neutralization of the luni-solar distorting power gives full sway to terrestrial re-formation, the results of which, although the converse of those we have just considered, are fully their equal in importance. These periods of maximum terrestrial action will result in a general increase of lateral pressures, tending to the compression of fissures and to the breaking of strata in a condition of stress. The increased pressure on the firepocket and lava-filled fissures of a volcano will force the magma to a higher level, bringing up fresh lava at a higher temperature and tending to cause lava flows; it will readily be seen how slight a compression of a fissure 40 km. in depth would be required to upforce a large amount of lava into the crater of a volcano. Powerful explosive effects may also accompany this phase, being due to the upforcing into the conduit and crater of active, high-temperature lava, and, although of a different character from the paroxysmal gaseous emission of the luni-solar maxima, these explosions may be more effective in rupturing the cone because acting against the pressure of a high lava column; they are, therefore, productive of lava flows and often initiate in this way the progressive reactionary process which will lead up to a catastrophal culmination coinciding with the succeeding luni-solar maximum. As the term *luni-solar minimum* would be confusing as applied to a condition which produces a positive effect, I prefer to designate this phase by the term "terrestrial maximum" as contrasted with "luni-solar maximum." We may, therefore, classify the two orders of maxima with their effects as follows: *Luni-solar maxima* equals *opposition* or *conjunction* plus *perigee* plus *declination,* tending to precipitate explosive crises, paroxysmal emission of gases and the culmination of great eruptions; and *terrestrial maxima* equals *quadrature* plus *apogee* minus *declination,* tending to cause lava flows, rupturing explosive effects and earthquakes. While earthquakes may be caused by either order of maxima, it will in general be found that the great tectonic earthquakes follow, as we should expect, a terrestrial maximum, while those of the volcanic or intervolcanic[4] type may succeed either order of maxima. Earthquakes lag, as a rule, a day or two behind the culmination of the maximum. As the two orders of maxima are complementary, it is obvious that the greatest possible effect will be produced when strong ones occur in proximity, *i. e.,* when a very favorable luni-solar maximum is followed by a very favorable terrestrial maximum, or *vice versa.* We may even suppose, given the general ascensional tendency of the volcanic magma, that a sort of pumping action may take place, a terrestrial maximum forcing lava upwards, a luni-solar maximum holding it there by gaseous expansion, a renewal of the terrestrial effect upforcing more lava, etc. It is possible that such an action may play an important rôle in the formation of new volcanoes, in the production of eccentric eruptions, in the reestablishment of communication between fire-pocket and crater through an obstructed conduit and especially, perhaps, in the eruptive processes of all trachy-andesitic volcanoes with their viscous, highly silicious magma. The compression and elongation stresses due to these alternating effects may also be a potent source of heat.

In plotting a curve of the two orders of maxima I have traced these above and below a medial zone representing an interval be-

[4] Mercalli thus classifies the Calabrian earthquakes, believing that these result from the movements of deep-seated magma.


LUNI-SOLAR MAXIMA
1902
TERRESTRIAL MAXIMA
JAN. FEB. MAR. APR. MAY. JUNE. JULY. AUG. SEPT. OCT. NOV. DEC.
LUNI-SOLAR MAXIMA
1905
TERRESTRIAL MAXIMA
JAN. FEB. MAR. APR. MAY. JUNE. JULY. AUG. SEPT. OCT. NOV. DEC.
LUNI-SOLAR MAXIMA
1906
TERRESTRIAL MAXIMA
JAN. FEB. MAR. APR. MAY. JUNE. JULY. AUG. SEPT. OCT. NOV. DEC.

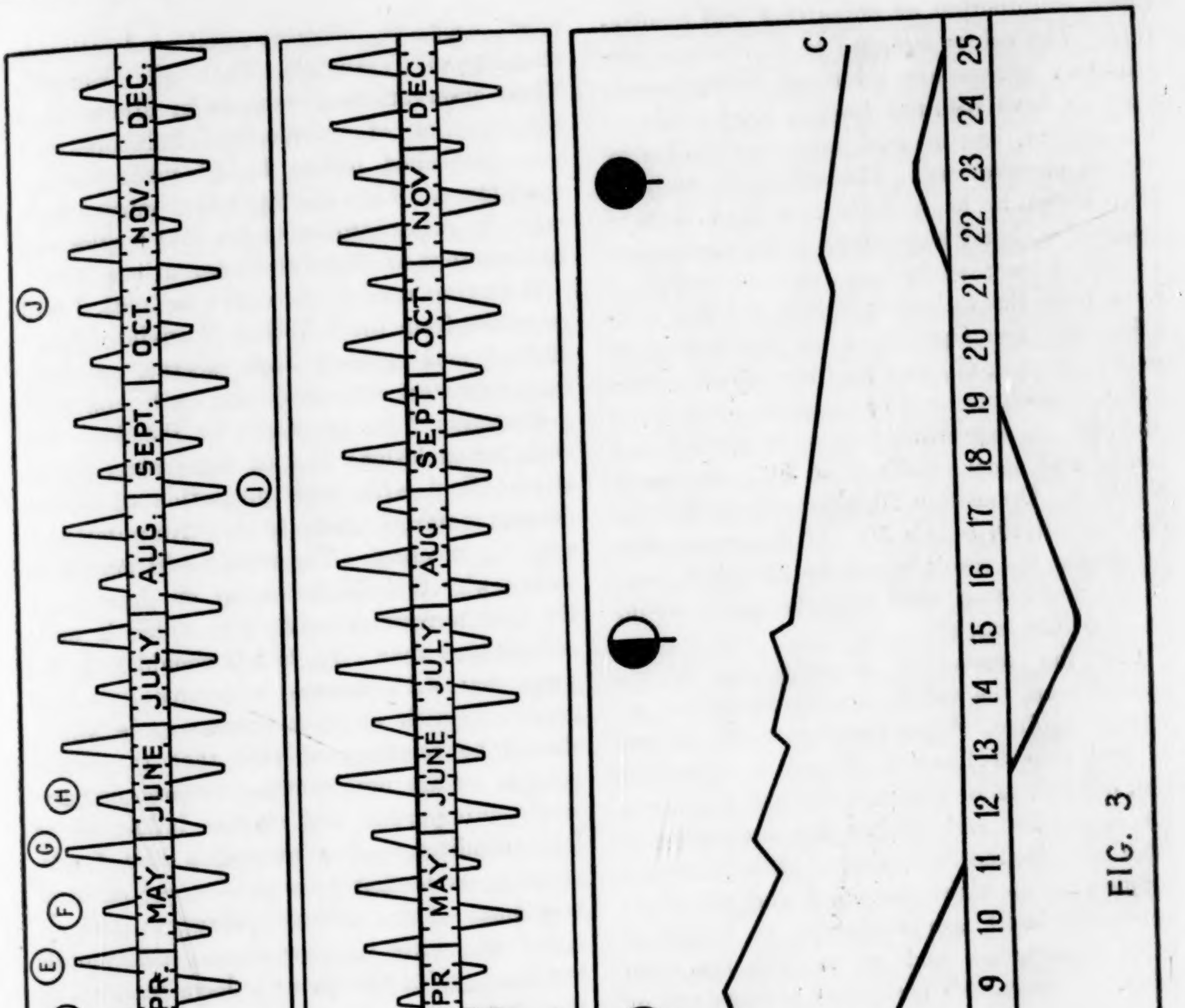

LUNI-SOLAR MAXIMA
1907
TERRESTRIAL MAXIMA
A
B
C
D
E
F
G
H
I
J
JAN. FEB. MAR. APR. MAY JUNE JULY AUG. SEPT. OCT. NOV. DEC.
LUNI - SOLAR MAXIMA
1908
TERRESTRIAL MAXIMA
JAN FEB MAR APR MAY JUNE JULY AUG SEPT OCT NOV DEC
C
D
APRIL 1906
30 31 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25

FIG. 3

tween conjunction or opposition and quadrature. The values assigned to the various contributory phases are arbitrary, being merely such as have resulted from a brief study of the subject, and as such may be modified by future observations. The effect of conjunction seems to be a little less than that of opposition, but I have given it the same value as the latter; perigee is very powerful. I have used the following values for the luni-solar maxima: Opposition or conjunction 20 points, perigee 20, and half the actual degree value of declination—for example: with moon and sun in opposition, moon in perigee and north and south declination 20°, the result would be: Opposition 20, plus perigee 20, plus declination 10, equals 50. If the moon were in apogee the result would be 20 points less; if the declination were zero the result would be 10 points less, etc.

For the terrestrial maxima, below the medial zone, the values are the same but inversely applied, thus: quadrature 20, apogee 20 and zero declination 10 points. Declination differences in alignment of sun and moon for opposition and conjunction are deducted from the declination values. Difference and supplement in right ascension and the right ascension itself are neglected as being of little importance, and no account has been taken of the earth's perihelion passage nor of the difference in power of sun and moon. The declination values of quadrature are very difficult to assign and it is here that most of the inaccuracy will be found—it is a problem for the astronomer. When any maximum occurs just before or after perigee the point of the maximum is inclined toward the date of perigee. The apogee-perigee values are taken from sinuous curves traced within the medial zone but which do not appear on the finished chart.

On the basis just outlined I have prepared the accompanying curves for 1902, 1905, 1906, 1907 and 1908. In comparing these with some volcanic phenomena occurring in these years I have preferred—in order to avoid any possible personal bias—to quote from the published observations of others, especially of that most accurate observer and indefatigable student of Vesuvius, Prof. G. Mercalli. These observations were made by him without consideration of astronomical influences and were published before I had made his acquaintance. The accompanying curves were made by me according to the above rules and in ignorance of Mercalli's observations.

Referring now to the chart for 1905, I will translate from his "Notizie Vesuviane (anno 1905)," making only such excerpts as relate to the greatest activity during each month.

January: "In the night of the 25th and 26th the explosions took on a character purely *strombolian*[5] with high projection of incandescent material (every four or five minutes)," see (A) on chart. The reader will here wish to ask why this did not occur on the date of the first terrestrial maximum, which is more accentuated than (A), but it should be noted first, that (A) follows a strong *luni-solar* maximum with which it forms a cycle, and it should be remembered also that the active periods we are now considering are comparatively unimportant and may be influenced by local conditions—the interesting and fundamental fact is that even these occur on some maximum of the curve. February: The activity was quite uniform during this month, but the culmination seems to have occurred on the 22d, when the incandescence at the crater—on other days spoken of as "sensible" —is parenthesized as "rather strong." This is close to (B). March: "On the 18th and 20th the incandescence was so vivid as to be visible as far as Naples at 6:45 P.M. while it was yet day . . . the new interclosed conelet . . . presented two points separated on the west by a profound depression, where, during the night of the 19th and 20th, I observed permanent incandescence due to a small outflow of lava or at least to the elevation of the

[5] Mercalli's use of this term, now generally adopted, signifies an emission of luminous materials, incandescent fragments and white vapors as contrasted with "vulcanian" which indicates the ejection of non-luminous blocks and bombs with abundant detritus presenting the appearance of black smoke. The terms are descriptive of the *character* and not the force of the explosion.

magma to the edge of the said depression," (*D*). May: In this month occurred the great event of the year—the lava outburst which initiated the long period of lava emission culminating in the catastrophal eruption of April, 1906. Mercalli writes: "In the night of the 25th–26th, after the strongest explosions, the entire surface of the terminal conelet was aglow. The *maximum* occurred on the 26th: by day I saw columns of white vapors without ashes which rose to a thousand meters above the crater; at 5 P.M., although yet day, already there commenced to appear the incandescence of the projected magma: later the ejected scoriæ formed streaks of fire on the external flanks of the cone. At 8:30 P.M. a strong explosion commenced with a very vivid *white light,* certainly due to flames; there followed after a few seconds the usual red color of the bits of incandescent magma. . . . During the day of the 27th the explosions and trembling of the ground were noticed as far as the Hotel Eremo. At about 5:30 P.M. the custodians of the upper station of the funicular railway felt strong earth-shocks. A little later, viz., at 6:15, there opened a first mouth of outflow," etc., (*E*). June: "About the 24th the increase of the outflow was accompanied by a strong explosive activity of the mouth where there was formed a conelet of scoriæ projected from a large eruptive fumarole, (driblet-cone of the English). . . . During the 23d and 24th the explosions were strong but mixed: in the evening they began with dark jets to which quickly succeeded the projection of incandescent scoriæ. On the 25th the explosive activity was very strong until evening (about 7 P.M.) when a portion of the interclosed terminal conelet collapsed," (*F*). July: "Often the lava flowed for a considerable distance covered by preceding lavas, then welled up again from 'pseudo-mouths.' Rapid changes succeeded from the breaking and perhaps the re-fusing of the lava crust at the times of increase. For example, in the early evening of the 29th the principal stream flowed for the most part covered; while instead, a few hours later (between 9 and 12 P.M.), I saw the stream all continuous and very vivid, especially in the lower part," (*G*). August: "The *maximum* occurred on the 8th: then the projectiles reached the edge of the crater of 1872 and the windows and doors of the lower funicular station were rattling," (*H*). September: "The morning of the 8th, between four and five o'clock (the lava) crossed the roadbed of the railway, covering it for about 120 meters, after having demolished in part the large stone wall which the firm of Cook had constructed to protect the lower station of the funicular," (*I*). October: The activity was generally great throughout the month, but I think the culmination is indicated by these words: "After 3 A.M. of the 28th–29th, very violent explosions commenced which shook the two stations, upper and lower, of the funicular." The station master wrote me: "After 3 A.M. a formidable first shock opened up a series of shocks which seemed as though each would dislocate the entire funicular," (*J*). November: . . . . "Slight increases (in the lava flow) occurred on the morning of the 6th (*K*) and on the 11th, 17th and 26th (*L*) (*M*) (*N*) . . . the explosions became rather strong on the 5th and 6th," (*K*). December: "After the 16th, a second, more central mouth gave mixed or vulcanian explosions: from the 16th to the 21st and especially on the evening of the 17th a considerable quantity of ash rained as far as the lower station of the funicular. All the while the strombolian action of the other mouth continued," (*O*). In a summing up for the year Mercalli states: "The elevation of the magma to the edge of the crater (19th–20th April) and the explosions accompanied by *flames* (26th of May) signal the two most important maxima of the strombolian dynamism," (*D* and *E*). "In coincidence with the second strombolian maximum there was instituted a sub-terminal outflow of lava," (*E*).

Mention should also be made of the great Calabrian earthquakes on September 8 (*I*) and on October 30 (*J*), and of a volcanic earthquake at Naples on November 26 (*N*).

Mercalli's "Notizie Vesuviane" for 1906 are not yet published. The activity was very great during the first months of the year, with

a marked increase in the lava output on February 2 (*B*)—chart for 1906—and an explosive maximum on February 7 (*C*), for an account of which see the New York Sunday *Herald*, April 1, 1906. The great Vesuvius eruption culminated on April 8 (*G*) and the reader is now referred to Fig. 3 which represents Mercalli's dynamic curve of the eruption —D. C.—to which I have added the astronomical data and the luni-solar and terrestrial maxima curve extended horizontally to fit the scale of Mercalli's diagram. Note how well the progressive reactionary process, of which we have spoken before, is exemplified in the dynamic curve from April 4, working up with increasing amplitude of vibration to the great culmination on the eighth, and note the general correspondence with the maxima curve throughout the entire eruption.

Assuming that we had had this luni-solar and terrestrial maxima curve at the beginning of 1906, it may be well to ask ourselves here if we could have predicted the eruption? Knowing the potential condition of Vesuvius at the time, we should probably have expected the eruption during the maxima combination (*C*) preceded by (*B*), but, that failing to be the crisis (although the activity was then very great), I think that we could have predicted it for April 8 (*G*), preceded as this was by a very strong terrestrial maximum. I have before pointed out that the length of the line joining two different maxima is a measure of the influence. In any event, we could have been morally certain that the eruption would have been precipitated by the strong maxima which follow—in other words, that the summer would not pass without a great eruption.

The San Francisco earthquake of April 18 showed the normal trifling lag behind the terrestrial maximum (*H*), but at Formosa an earthquake occurred on April 14, and another on March 17 (*F*). Other severe earthquakes occurred during the year in Iceland, November 8, 9 (*N*) and November 22 (*O*); Kopal December 22 (*P*); Sicily September 13 (*M*); Calabria January 17 (*A*) and June 10 (*J*); Central Asia August 13 (*L*); India March 10 (*E*) and July 21 (*K*); Columbia January 31 (*B*); West Indies February 21 (*D*); and Saxony April 28 (*I*).

On the curve for 1907 the five explosive crises of the great eruption of Stromboli are shown at (*D, E, F, G, H*), for a full account of which, with dynamic curve, the reader is referred to the *Brooklyn Institute Museum, Science Bulletin*, Vol. 1, No. 7. Mauna Loa was in eruption January 10 (*A*) and Jaggar reports a violent disruptive explosion of Bogosloff on September 1 (*I*). Severe earthquakes occurred at Jamaica, January 14 (*A*), and March 25 (*B*); in Mexico, April 14 (*D*); Bitlis, March 31 (*C*); San Miguel (Azores) April 2 (*C*); and in Calabria, October 23 (*J*).

In order to look backward a little I have selected 1902 as being rich in volcanic manifestations and have prepared a curve for that year. The great explosive crises of Pelée occurred on May 8, June 6, July 9, August 30 and December 16. Professor Lacroix writes ("La montagne Pelée et ses éruptions") "Le 8 Mai s'est produit le phénomène terrifiant qui, en quelques minutes, et peut-êtres moins, a anéanti S. Pierre et ses 28,000 habitants" (*D*). Nuées ardentes 20 Mai (*E*) et 6 Juin (*F*). "Une brusque recrudescence paroxysmale le 9 Juillet" (*G*). "Le 24 Août une secousse de tremblement de terre est ressentie dans toute l'île" (*H*). "Un grand paroxysme se prepare, il éclate le 30 Août" (*I*). Nuée ardente 16 Decembre (*M*).

At St. Vincent: "A la fin d'Avril 1902 ils (earthquakes) augmentèrent, . . . le 29 Avril il ne se produisit pas moins de 18 secousses au Morne Rouge (*C*)." "Le 7 Mai eut lieu la grande explosion (*D*)." "Un nouveau paroxysme s'est produit du 15–16 Octobre" (*L*).

The Pelée paroxysm of July 9 does *not* correspond with the curve. The writer would call special attention to this, the only notable exception in the entire series of events, in the hope that some explanatory observation may be forthcoming which shall give greater precision to the making of future curves.

At Izalco (Salvador) there was a violent eruption September 29 (*K*).

Heilprin ("Mt. Pelée and the tragedy of Martinique") mentions severe earthquakes in Guatamala, April 18 (*B*) and September 26 (*J*); Finland, April 10–11, and Lake Baikal, April 12 (*A*); Caucasus, April 17 (*B*).

Finally, to turn from the past to the present and future, I have plotted the curve for 1908—a clean page upon which the reader may make his own observations. The Chilapa (Mexico) earthquake and the sudden disappearance of the lake in Oregon will be found to correspond well with the curve. The earthquake registered by the Washington seismograph May 15 and calculated to have occurred at a point about 3,000 miles distant should, if our conclusions are correct, be of volcanic origin, corresponding, as it does, with a luni-solar maximum. From the condition of Etna during 1907 the writer freely predicted an eruption during the present year and, with the aid of the curve, localized it to the spring and summer months. At the time of writing (May) news has come of the initiation of the eruption and it will be interesting to follow its course in connection with the curve and to see if its crises and culminations correspond with the maxima of June 15, July 14 or August 13.

In conclusion the writer desires to state that he fully realizes the crudity and incompleteness at present of this working theory, and his object in bringing it forward at this time, instead of elaborating it by further study and observation, is to stimulate the criticism of others in order that the truth may be the more rapidly advanced. The importance of being able to foreknow the dates in each month when volcanic and seismic manifestations will take place is too obvious to require emphasizing and these data, in connection with research work localized at volcanic and seismic centers, should carry us a long step forward along the line of definitely predicting all such events. During the past two years the writer has often made use of this foreknowledge in planning his visits to volcanoes at interesting times and in absenting himself for preparation work during the intervals of quiet, and it was principally by means of the luni-solar curve that the crisis in the eruption of Stromboli last year was shown to have already occurred when warships had been sent with a view to deporting the 4,000 inhabitants. A resort to this extreme measure was thus rendered unnecessary and this application of our working theory forms a good example of its practical utility.

The present activity of Etna should form a good control and will undoubtedly be of aid in the computing of future curves.

FRANK A. PERRET

THE LOCATION OF EMBRYO-FORMING REGIONS IN THE EGG

THE relation between the visible substances of the egg (nucleus, yolk, pigment, oil, etc.) and the regions of organ-formation has attracted the critical attention of embryologists in recent years. No little diversity of opinion has been expressed as to the rôle played by these substances; whether they represent organ-forming regions, or whether they are only indicia, at most, of more profound changes, is at present the central point of dispute. The separation and stratification of many of these substances by means of the centrifuge has made possible the further analysis of the problem. I wish to put on record here the results of an experiment that bears very directly on the interpretation of the location of organ-forming regions of the egg of *Arbacia*.

As first shown by Lyon, the egg of the sea-urchin may be stratified into four regions by means of the centrifuge. The nucleus is driven into the axis of rotation (secondary egg-axis) and comes to lie near the lighter pole of the egg. Cleavage takes place in most cases at right angles to the stratification. I have been able to demonstrate that the cleavage planes stand in no relation to the original egg axis. Nevertheless, the typical cleavage system generally appears. The primary axis of the embryo, however, bears no fixed relation to the stratification. The fundamental question to settle therefore is what factor determines the location of the embryonic axis.

This point I have been able to determine by means of the "attachment funnel" (micropyle?) of the egg-membrane. Boveri has shown that the funnel corresponds to the point of attachment of the egg to the wall of the ovary. It lies opposite to the point of formation of the micromeres in the normal egg, and therefore also opposite to the gastrula pole of the egg. I find that in the centrifuged egg of *Arbacia* the micromeres also appear opposite (or as nearly so as possible) to the attachment funnel, without regard to the stratification of the materials.

Miss G. B. Spooner, working with me, has demonstrated that wherever the micromeres lie on the centrifuged egg there also the gastrulation takes place. Putting together these facts, it is evident that the axis of the embryo derived from the centrifuged egg is the same axis as that of the normal egg. In other words, the location of the nucleus, of the oily matter, of the yolk, and of the pigment of the egg has no determinative influence on the location of the embryonic organs. These visible materials are not organ-forming, nor do they act as initiators of organ formation. Even removal of the nucleus from its normal relations to the egg-axis has no baleful influence on the development.

The results demonstrate that the location of embryo-forming regions is a cytoplasmic and not a nuclear phenomenon. Boveri's classical experiment with dispermic eggs, which he brought forward in order to demonstrate the importance of the nucleus in the early development, receives an entirely different interpretation in the light of these facts. The conclusive demonstration of the location of the primary axis furnished by the experiment given above leads to some far-reaching conclusions concerning the factors of development, and the supposed value of the grosser materials of the egg as organ-forming substances:

1. By means of the centrifuge it is possible to drive the nucleus, the yolk, and pigment granules through embryo-forming materials of the egg without necessarily affecting the polar relations of these materials, and without necessarily injuring them for further development.

2. The displaced nucleus does not return to its original position before cleavage, and its new location determines the position of the first plane of cleavage. There is no essential relation between this plane of division and the planes of the embryo.

3. The embryonic axis is determined in the egg, but whether it is the outcome of an arrangement or gradation of materials which are not affected by the centrifuging, or whether it is due to a hidden structural basis ("organization") can not perhaps be determined from the evidence. When all the facts are taken into consideration, however, the former alternative seems to be more in accord with the results.

4. After centrifuging and before cleavage there is to some extent a remixing of the separated substances, but this partial return shows no evidence of redistribution in the direction of subsequent organ-formation, but is due to movements connected with karyokinesis.

5. The possibility that formative substances are present other than the visible substances here referred to must, of course, be admitted, but such materials are in the egg of the sea-urchin not seriously disturbed by a centrifugal force sufficient to separate the visible substances of the egg. On the other hand, I have found for the egg of the frog that a speed higher than that necessary to separate the grosser materials interferes with the normal development, and in such cases it seems not improbable that more fundamental materials become displaced.

6. The experiments do not show conclusively the origin of the bilaterality of the embryo, but they do show that this is not caused by the stratification, *nor by any particular cleavage plane,* nor by the position of the nucleus.[1] The inference that bilaterality is also "given" in the egg seems therefore most plausible.

T. H. MORGAN

WOODS HOLE,

August 17, 1908

[1] The evidence on which this statement rests is only partly given in the present communication.